

the writer had established his case in 1662. As to this there can hardly be two opinions; and Prof. Tait is fully justified in insisting upon his objections to "Mariotte's law." In Appendix IV. a curious passage from Newton is discussed, in which the illustrious author appears to speak of Mariotte sarcastically. It is proper that these matters should be put right; but Prof. Tait is hardly impartial enough himself to succeed in enlisting the complete sympathy of foreigners. Cases of glaring injustice should be rectified; but there will always be a tendency (from which Englishmen cannot claim to be exempt) to give a full measure of credit to one's own countrymen, if only because one is better informed concerning their labours.

There is one matter, suitable to an elementary work, which I should be glad to see included in a future edition, viz., the principle of dynamical similarity, or the influence of *scale* upon dynamical and physical phenomena. It often happens that simple reasoning founded upon this principle tells us nearly all that is to be learned from even a successful mathematical investigation; and in the very numerous cases in which such an investigation is beyond our powers, the principle gives us information of the utmost importance. An example will make this clear. The pitch of a tuning-fork of homogeneous steel is dependent upon the size and shape as well as upon the elastic quality of the material; but the matter is too difficult for rigorous mathematical treatment. If, however, it be asked, How does the pitch depend upon the *size* of the fork, the shape and material being given? we need no complicated mathematics at all. The principle of dynamical similarity tells us at once that the time of vibration is proportional to the linear dimension.

Another example might be taken from a reaction which Prof. Tait describes as specially complex—viz., collision. A glass ball drops upon a marble floor from a height of one foot. How does the size of the ball affect the strains during collision and the danger of rupture? The principle teaches that if the scale of time be altered in the same proportion as the scale of length, similarity is secured, so that the strains are equal at corresponding times and at corresponding places. Hence a larger ball is not more likely to break than a smaller one, unless in consequence of the greater *duration* of the strains. I feel sure that in Prof. Tait's hands this very important and fundamental principle might be made intelligible to the great mass of physical students.

It would lead us too far to refer in detail to the various subjects treated in the later chapters under capillarity, diffusion, osmose, transpiration, viscosity, &c., but there is one point that I should like to mention. The explanation on p. 249 of the behaviour under water of drops of ink and of solution of permanganate of potash assumes the existence of a capillary tension in the surface separating the two fluids. In my own experiments on jets with this very solution, I have never seen any tendency to break up into drops (as, according to Savart and Plateau, there would be in air), and have therefore supposed that the capillary force was *nil*, or at any rate very small. Moreover, theory shows that the force depends entirely upon the suddenness of transition between two media, which suddenness must be broken down almost instantaneously when two miscible liquids

come into contact. As the matter stands there seems to be here some discrepancy, which, perhaps, Prof. Tait could elucidate.

In his preface the author holds out hopes of further volumes on the same plan, dealing with dynamics, sound, and electricity. The readers of the present work will, I am sure, join in the wish that the appearance of these may be delayed no longer than is absolutely necessary.

RAYLEIGH

GRISEBACH'S "VEGETATION OF THE EARTH"

Die Vegetation der Erde nach ihrer klimatischen Anordnung. Ein Abriss der vergleichenden Geographie der Pflanzen. Von A. Grisebach. Zweite vermehrte und berichtigte Auflage. 8vo. Vol. I., pp. 567; Vol. II., pp. 693. (Leipzig: Wilhelm Engelmann, 1884.)

FROM the date, and the statement on the title-page that this is an augmented and corrected second edition of a work which was published in 1871, it might be expected that it contains the results of much more recent investigation; but an examination of the present edition is very disappointing. Indeed, it is doubtful, to say the least, whether it deserves the descriptive title given to it; for the "Quellenschriften und Erläuterungen" do not appear to contain a single additional reference, and it is not easy to discover that it has a claim to be anything more than a reprint, with some trifling alterations, of the original edition of 1871. The author died in 1879, so that one naturally looked to see who was the editor of this edition, and it was only after much seeking that a clue was found in a foot-note on p. 15 of the preface. After the appearance of the "Vegetation der Erde," A. Grisebach continued to write annual reports on the progress of geographical botany, and these, together with other scattered articles, were published in a collective form in 1880 by his eldest son, under the title, "Gesammelte Abhandlungen und kleinere Schriften zur Pflanzengeographie." From the foot-note in question it appears that this son—a gentleman in the Consular service of his country, and presumably unacquainted, or imperfectly acquainted with botanical literature—edited the new edition of the "Vegetation der Erde," "based upon the corrections and additions left by the author." Now it is perfectly certain that Grisebach regarded the annual reports referred to as so many supplements to his greater work, and the substance of which he would doubtless have incorporated therein had he himself prepared a second edition. Since his death, too, considerable additional information on geographical botany has come to light; and, what is more, it has been collected and published in German by Drude, Engler, and others; yet, as already mentioned, the additions and corrections in the present edition are merely trivial, and cannot be said to enhance materially the value of the work. In a foot-note to Grisebach's preface to Tchihatcheff's admirable French edition of the original work, reproduced here, it is stated that some additions of Grisebach's thereto are here intercalated in their respective places. This is very good, but why Tchihatcheff's copious annotations and additions, recognised and sanctioned, as it were, by the author himself, should be ignored in a second German

edition, is incomprehensible, saving the assumption that both with respect to his father's annual reports and other sources, the son was wholly incapable of doing his father justice. It is a pity that the task of preparing a second German edition was not entrusted to a competent botanist, because the original work, apart from the uncompromising antagonism to Evolution that pervades it, still occupies an undisputed position in modern botanical literature. As it is, the French edition is not merely an advance on the original German—it is incomparably better than the second German edition. It is only, however, fair that some justification of such assertions should be given. Taking the chapter on Oceanic Islands as an example, it may be confidently stated that no additional information is given; yet there is no branch of geographical botany that has advanced more during the last decade than insular. On the other hand Tchihatcheff embodies nearly all that was known up to date. One slight alteration observed in this chapter is—Madeira is stated to be 50 German geographical miles nearer Europe than the Azores, instead of 150, as in the original. Then certain unfounded statements in refutation of the arguments of other botanists concerning the relationships of insular floras remain uncorrected. Thus, in allusion to Sir Joseph Hooker's demonstration ("Insular Floras," p. 7) that the vegetation of St. Helena has, on the whole, its nearest affinities in South Africa, it is objected, on the authority of Roxburgh, that three out of the five genera named by Hooker were originally introduced into the island from the Cape of Good Hope, whereas an examination of Roxburgh's enumeration of the plants of St. Helena reveals the fact that the indigenous, and endemic, St. Helena species of the genera in question were unknown to him, and his remarks apply only to actually introduced species. Again, to repeat in 1884 such statements as that the vegetation of Juan Fernandez has little systematic relationship with that of the Chilian or Antarctic floras and that *Pringlea anti-scorbutica* is restricted to Kerguelen Island is unpardonable, because the contrary is now historical. Defects such as those pointed out are numerous, but as they are mostly due to the state of knowledge fifteen years ago, the author of the work of that date is not to be blamed for them; rather the present editor and publisher for offering the public an old book as new.

W. BOTTING HEMSLEY

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Nomenclature in Elasticity

THE word *stress* is used, sometimes in the sense of *load*, sometimes in that of *load per unit area*. Clearness, however, requires these two ideas to be kept perfectly distinct, and therefore to be denoted by separate terms. *Load* is surely expressive enough, or, if not, there is the more comprehensive word *force*: why then use *stress* synonymously? It would be far better to reserve *stress* to signify *load per unit area*. This Prof. Kennedy (p. 269) calls *intensity of stress*; but why not *stress* simply? The

word *intensity* is not in itself suggestive of anything distinctive, and is therefore useless.

Pressure and *tension* are terms used in the same loose manner, though, when intended to represent *force*, they sometimes have the word *whole* prefixed. Is it not better to say *force* when we mean *force*? We can then reserve *pressure* and *tension* as vector-synonyms of *stress* in the sense of *force per unit area*, which is indeed their usual rôle.

Another misused term is *resilience*, which sometimes denotes the *work* done in producing *proof strain* in a body (Rankine's definition), sometimes the *work done per unit volume* in producing *proof strain*, sometimes the *work done per unit volume* in producing *any strain*. I prefer, myself, the third definition; the second would then be the *proof resilience*, and the first might be called the *strain-energy*.

However, whatever terminology is finally agreed upon, let it be perfectly definite and consistent.

In his Fig. 1 (p. 269) Prof. Kennedy writes: "Breaking load, 18.85 tons per square inch." According to his own nomenclature, he should surely say: "*intensity of breaking stress* 18.85 tons per square inch," and this I should prefer to call simply the breaking stress—premising that for *tons* I should write *tons' weight*. In this case, as the diameter is $\frac{3}{4}$ inch, and therefore the section .442 square inch, the breaking *load* is 8.33 tons' weight. Similarly in the other figures.

Christ Church, Oxford

ROBERT E. BAYNES

Earthquake-Proof Buildings

MR. MUIR is quite correct as to the facts and date of the introduction of the aseismic tables into Japan. In 1869–70 seven aseismic tables for carrying the lighting apparatus were sent from here and erected in Japan, and Mr. Simpkins, who has recently returned from Japan, informs me that there are three in action at present. Two iron towers, 46 feet high, with this arrangement at their base, were also constructed and shipped for Japan, but the vessel was lost and no more were sent out, as the engineer in charge—Mr. Brunton—took an unfavourable view of their efficiency—his idea being that they would not work, as he considered that buildings of "great weight and solidity, thereby adding to their inertia and checking their oscillation, were best suited to meet the difficulty in Japan." Mr. Milne's experiments with aseismic tables have borne out Mr. David Stevenson's original view as to their power of mitigating an earthquake shock. For fuller information see NATURE, vol. xxx. p. 193.

Edinburgh, August 3

D. A. STEVENSON

A Mechanical Telephone

HAVING observed in this week's NATURE a notice of a "mechanical telephone" said to be brought from America, I may state that so far back as 1878 I experimented on the transmission of sounds by wires, and communicated the results obtained, from a large number of experiments, to the Physical Society of London in March, 1878; the paper being afterwards published in the *Philosophical Magazine* for August, 1878. These experiments are referred to by the Count du Moncel in his book on "The Telephone," published in 1879. I found no difficulty in carrying on a conversation through wires laid in various ways from room to room of a house; and musical sounds, breathing, and whistling were also readily transmitted, and through most unlikely arrangements, such as a common wire fence. Various materials were tried for the transmitting and receiving ends—disks of cardboard set in deepish rims being found to give excellent results with a No. 16 copper wire. In one of my experiments I found that the disks were not required, the wire itself picking up and transmitting the sounds. The results obtained were most interesting; but as the range was necessarily limited, it did not seem to me that there was much scope for practical application.

W. J. MILLAR

100, Wellington Street, Glasgow, July 31

Electrical Phenomenon

ABOUT ten o'clock in the evening of July 23 a party of four of us were standing at the head of the avenue leading to this house, when we saw a feebly-luminous flash appear on the ground at a distance of some thirty yards down the avenue. It rushed towards us with a wave-like motion, at a rate which I estimate at thirty miles an hour, and seemed to envelop us for an instant.